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On Ancient Eclipses. By P. H. Cowell.

Two papers have lately appeared in the *Monthly Notices* in criticism of the conclusions that I have drawn from ancient eclipses.

I have read Mr. Nevill's paper with the greatest possible interest: he takes the standpoint that the question is one to be settled by the evidence, and not by preconceived theory, and he brings forward a large number of additional eclipses with which he tests my conclusions. I intend, therefore, to perform as soon as possible my own calculations upon these eclipses. I shall supplement Mr. Nevill's results by giving the least distance between the centres of the Sun and Moon as seen from a specified place, with symbolical corrections for the unknown quantities whose values are to be determined. At the present time, before these calculations are ready, I can offer only a few general remarks.

1. I did not choose the date −647 for the eclipse of Archilochus, but found it already assigned, and I adopted it without further inquiry. I now admit the force of Mr. Nevill's reasoning, and the eclipse must therefore be relegated to the same category as the eclipse of Agathocles.

2. Sivan was the third Babylonian month. If, therefore, July 31 was the end of Sivan, the year must have begun about May 4, and the month that began on April 4 was presumably an intercalary month. The equinox fell about March 31, and the difficulty is, therefore, only one of a few days, and not of one or two months, and consists in the question, Why was April 4 not recognised as subsequent to the equinox? But in any case errors in the calendar which might be impossible a few centuries later are not necessarily inadmissible at an earlier time. I understand that Mr. Nevill's alternative of relegating the eclipse to the twelfth century does not find favour with Mr. King. Mr. Maunder is sending an interesting note on this question to the July number of *The Observatory.*

3. Thucydides was an Athenian writing principally for Athenians. I consider the probability great that he either saw the eclipse at Athens himself, or that—if the phenomenon was different at Athens from what he described—some of his friends would have told him so.

4. In the case of the first twelve eastern eclipses given by Mr. Nevill I find in *M.N.* xxiv. p. 42, the statement that the eclipses of −708 and −600 are alone described as total. For these two eclipses, as well as for those of −180, −187, and −299, subsequently given by Mr. Nevill, my system does very well, and distinctly better than any of Mr. Nevill's three systems. The eclipse of −381, as pointed out by Mr. Nevill, fits no system of corrections.

5. Mr. Nevill makes no allusion to the lunar eclipses of the Almagest, which support my conclusions.

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6. I have, of course, referred to the observations of the Sun during the last 150 years. It goes without saying that I do not maintain the value $4''\cdot 1$, which I find for the secular accelerations of the Sun is correct to $0''\cdot 1$. A very slightly smaller secular term might easily be masked by the errors of personality that we know to exist. I have still the further loophole that there may exist an unknown term of 300 years' period and coefficient $1''$ in the Sun's longitude. (If the unknown term in the Moon's longitude is to be attributed, as Professor Newcomb once supposed, to an inequality in the Earth's rotation, a term of the required period, amplitude, and phase must necessarily exist in the Sun's longitude.)

7. I should like to raise the question as to how eclipse calculations should be presented. I prefer to resolve in longitude and latitude, at right angles to the line of sight. Mr. Nevill and many others resolve along the surface of the Earth, and tell us that according to certain tables the central line passed so many degrees north or south of a certain place. This mode of presentation is complicated by the effects of foreshortening, and we cannot tell, without considerable labour, what would be seen from the place chosen for calculation. It is obvious also that it is far easier to carry through suggested modifications of the tables in my mode of presentation than in Mr. Nevill's. I should also like to ask whether it is not worth while to give some intermediate steps in the calculation. In my first paper on solar eclipses in five pages I gave for five eclipses full formulæ and a great many intermediate steps in the calculations. Anyone who wishes to repeat my calculations can do so without any further work of reference than a table of logarithms. He can see clearly what terms I have retained and what I have neglected as unimportant. At intervals, in no case exceeding half an hour, he can say, as the case may be, "So far the calculations are numerically correct" or "An error of calculation has been introduced at this point." Lastly, if he wishes to modify my secular accelerations or other parts of my formulæ, he can do so in a few minutes. On the other hand it is impossible for me to supplement Mr. Nevill's calculations without repeating his entire work from the beginning. I very much regret this, as I have found his paper most interesting, and I deplore the unnecessary delay before I can make full use of it.

Professor Newcomb has now published a second note. It must be obvious that neither of these notes contains the features which render Mr. Nevill's paper so valuable. I find his position as to the evidence extremely difficult to understand, for he begins by saying that part of the evidence is absolutely uncertain, and he ends by asking whether it is sound not to have used more of it. I picked out what in my judgment was the best evidence, and I found it accordant upon an excessively simple geometrical hypothesis.

I need not follow him in detail through all the eclipses. I

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should never have thought of basing my conclusions on any two of them. I base my conclusions on the coincidence of all of them. A proof from coincidence is like circumstantial evidence in a criminal trial. It is unnecessary when enough direct evidence is to be had, but it is often of great force.

As for the lunar eclipses, there should be no great difficulty in estimating the magnitude to the nearest tenth of a diameter, with a probable error of one-fortieth. In ten cases magnitudes are recorded. In nine cases they indicate a large correction to the distance between the node and the Sun, always of the same sign and averaging over 1000″.

In nine of the nineteen lunar eclipses no numerical estimate of magnitude is on record. They are therefore useless for my special purpose. No. 8 is one of these. In this case Professor Newcomb has accidentally interpolated the words "at Babylon" in what purports to be a translation of Greek. No such words occur in the Greek. Other less important discrepancies between Greek and English occur in Nos. 16, 17, and 18. It does not follow that I am unaware of a mistake because I have not alluded to it, more especially as in this case eclipse No. 8, and everything connected with it, is irrelevant to the main issue.

The calculations referred to at the beginning of this note are by now sufficiently far advanced for me to make the following statements :—

1. The eclipses of −1123, −1116 and −1069 cannot possibly (that is to say, by any moderate alterations of the tables) have been total at Babylon. If, therefore, "fire in the midst of heaven" implies a total eclipse at Babylon, the date is −1062.

2. The eclipse of −609 cannot have been total at Larissa; and the eclipse of −602 was total at Larissa according to my formulæ.

3. The eclipse of −584 will do perfectly well for the eclipse of Thales. According to my formulæ, the time of maximum eclipse occurred a few minutes before sunset.

Note on Polarisation Phenomena in the Solar Corona, 1905 *August* 30. By H. F. Newall.

The present note is presented in compliance with a request from the President. It contains an attempt at a brief *résumé* of the chief results of the observations made by the expedition of which I was in charge at Guelma relating to the phenomena of polarisation of the corona of 1905 August 30, so far as they can be stated apart from the complete measurement and study, which I hope to be able to make during the summer, of the photographic records.

It has, as I gather, been felt that the results of eclipse expeditions as given in the preliminary reports of observers are in